Theory and Policy in International Relations: Some Personal Reflections

By Stephen M. Walt

I. INTRODUCTION

Most social scientists would like to think that their work helps solve important problems. For scholars of international relations, there is certainly no shortage of issues to address: ethnic and religious conflict, managing a fragile world economy, global terrorism, climate change, the spread of weapons of mass destruction, the Euro crisis, etc.—the list is endless. In this increasingly complex and still-contentious global order, one might think that scholarly expertise about international affairs would be a highly valued commodity. One might expect to see academic theorists working overtime to devise practical solutions to various real-world problems and playing prominent roles in public debates about foreign policy. Yet this does not seem to be the case for most of them. Former policy makers complain that academic scholarship is “either irrelevant or inaccessible. . . locked within the circle of esoteric scholarly discussion,” and one academic recently charged that “scholars are focusing more on themselves, less on the real world. . . Inquiry is becoming obscurantist and in-grown.”¹

This situation is not what I anticipated when I decided to pursue a PhD in political science in the spring of 1976, while studying at Stanford University’s overseas program in Berlin, Germany. My undergraduate major was International Relations, and I was torn between graduate study in political science or the more well-trodden and risk-averse path to law school. A lecture on Weimar-era intellectuals by historian Gordon Craig tipped the balance: Craig argued that many German intellectuals had withdrawn from public life during this period—deeming politics too corrupt and sordid for their enlightened participation—and their abdication had helped open the door to Nazism.² Young and idealistic (some would say naïve), I decided to get a PhD and try to bring scholarship to bear on important public policy issues.³

It has been nearly thirty years since I received my PhD. At that time, I was convinced that systematic scholarly research could uncover and verify timeless truths about international politics and foreign policy, and that once those discoveries had been made,
grateful policy community would quickly absorb them and adopt the right prescriptions. With the passage of time, I’ve gained both a greater respect for the limits of what social science can accomplish and a greater appreciation for the imperviousness of the policy community to reasoned discourse, especially in the United States. Even if scholars were able to produce more convincing analyses— itself a debatable proposition—overcoming the entrenched interests that shape what policy makers choose to do is not easy.

This theme can be traced through my own work, although it did not shape my scholarly path in any conscious way. My initial work on alliance formation (e.g., *The Origins of Alliances*, 1987) was intended to resolve some theoretical puzzles that lay at the heart of recurring policy debates about the use of force in US foreign policy. I argued that the claim that states were inclined to bandwagon (i.e., ally with strong and/or threatening powers) was often used to justify the use of force, largely to maintain US credibility and prevent allies from defecting toward the Soviet bloc. By contrast, if states were inclined to balance against threats, then US credibility was not as important and fighting costly wars in the periphery was not necessary. US intervention was also justified by the perceived need to prevent left-wing governments from gaining power, based on the belief that such regimes were ideologically disposed to ally with Moscow. My research showed that balancing was much more common than bandwagoning, and my primary policy conclusion was that because the United States enjoyed enormous geopolitical advantages over the Soviet Union, it did not need to intervene in the developing world for credibility reasons and could generally take a much more relaxed view of its security requirements. The book was well-received in the academic world and attracted some modest attention within policy circles, but it is hard to discern any direct effect on US foreign policy.

A subsequent work (*Revolution and War*, 1996) applied balance-of-threat theory to explain why domestic revolutions led to increased security competition and a heightened risk of war. Once again, it began with a policy puzzle: why were US policy makers so alarmed by most domestic revolutions, and why did Washington have such poor relations with revolutionary Russia, China, Cuba, Iran, and several others? I found that revolutions made calculating the balance of power more difficult, unleashed mutual misperceptions that made the use of force seem both necessary and attractive, and usually led to heightened levels of security competition and an increased risk of war. I argued that strategies of “benevolent neglect” were likely to dampen these effects and enable the United States (and others) to contain the effects of revolutionary upheavals at less cost and risk. Whatever the merits of these arguments, evidence of policy impact was slim to non-existent.

In *Taming American Power: The Global Response to US Primacy* (2005), I sought to explain how friends and foes were responding to the unusual position of dominance that the United States enjoyed following the demise of the Soviet Union. Why were even long-time US allies alarmed by US primacy, and what strategies did allies and adversaries employ to deflect US power or to exploit it for their own ends? Although not limited to purely realist concepts, this work nonetheless reflected a basically realist sensibility: even if US foreign policy were motivated by noble aims, other states could not take US benevolence for granted. To reduce opposition to US primacy and ensure
that key US allies bore their fair share of collective security burdens, I argued for a grand strategy of “offshore balancing” that would reduce the global military footprint of the United States and avoid long and costly wars in areas of marginal strategic importance. The case for this prescription is even stronger in the wake of the 2007 financial crisis and the failed campaigns in Iraq and Afghanistan, but it is these events that are pushing the United States toward a smarter grand strategy and not my earlier eloquence.

Finally, my work with John Mearsheimer on the impact of the Israel lobby was both a departure from purely realist analysis and one that nonetheless reflects our shared realist roots. In our view, the “special relationship” between the United States and Israel is not in either state’s long-term strategic interest and is thus inconsistent with basic realist principles. For realists, therefore, the lavish and unconditional support that the United States provides to Israel is an anomaly that needs to be explained. We argued that it is accounted for primarily by the influence of particularly powerful set of interest groups in the United States. The book was a best seller and helped open up a long-overdue debate on this issue, but both the lobby’s influence and the special relationship itself appear largely unaffected thus far.

What have these and other experiences taught me about the relationship between theory and policy? The first (and somewhat depressing) lesson is that academic theory—including my own work—has had relatively little direct or indirect impact on actual state behavior. Scholars may tell themselves they are “speaking truth to power,” but most of the time the powerful don’t listen. To note an obvious example with which I was personally associated, the effort by two prominent groups of security scholars to oppose the decision to invade Iraq in 2003 had no discernible impact on the Bush administration’s march toward war, or on the many Democrats who eagerly supported Bush’s action.

Why is academic writing on foreign affairs of such limited relevance? To answer that question, let us first consider what theory might be able to contribute, and then consider why its impact is relatively modest.

II. THE ROLE OF THEORY IN THE POLICY PROCESS.

We live in a world of dizzying complexity. Each day, policy makers must try to figure out which events most merit attention and which items can be deferred, and they must select longer-term objectives and choose policy instruments they believe will advance them. To do this, they depend on purely factual knowledge (e.g., What is the current balance of payments? How much enriched uranium does Iran have?) but also on simple typologies (e.g., “revisionist” versus “status quo” powers), on “rules of thumb” derived from experience, or on well-established empirical laws (e.g., “Democracies don’t fight each other”). And whether they are aware of it or not, policy makers invariably use explicit or implicit theories that purport to identify causal relations between two or more variables of interest.
Because contemporary IR theories are relatively weak and definitive empirical tests are elusive, policy debates often hinge on competing theoretical claims. In the 1990s, for example, disagreements over how to respond to the Balkan wars rested in part on competing theories about the causes of ethnic strife.\(^\text{12}\) Today, competing prescriptions over how to deal with China’s rise rest in part on rival theories of world politics, with realists favoring preventive actions designed to contain Chinese ambitions, liberals advocating policies of engagement designed to foster ties of interdependence, and social constructivists seeking to “socialize” China within existing norms and institutions.\(^\text{13}\)

These debates are important because relying on bogus theories can get states into deep trouble. Prior to World War I, German admiral Alfred von Tirpitz’s “risk theory” argued that naval expansion would put the Royal Navy at risk and deter Great Britain from opposing German ambitions. Instead, this policy led the British to align more closely with Germany’s enemies. The infamous “domino theory” helped justify America’s costly involvement in Indochina and its ill-advised interventions in Central America, just as the neo-conservatives’ naïve beliefs about the ease with which democracy could be spread via military force paved the way toward disaster in Iraq.

The converse is also true, of course: good theories often produce beneficial policy results. The Ricardian theory of free trade helped increase global economic growth, and the theory of nuclear deterrence developed in the 1950s informed many aspects of US defense policy and almost certainly reduced the danger of nuclear war.

From a policy maker’s point of view, what is a good theory? A good theory should be logically consistent and empirically valid (i.e., it should fit the available evidence), it should also help policy makers comprehend phenomena that would otherwise be incomprehensible. (This is what we mean by a theory’s “explanatory power.”)\(^\text{14}\) Theories are more useful to policy makers when they deal with important phenomena, and when they contain variables over which policy makers have some leverage.\(^\text{15}\) Finally, theories are most useful when they are stated clearly. _Ceteris paribus_, a theory that is hard to understand takes more time for potential users to grasp and is usually harder to verify and test.

How does theory help policy makers do their jobs more effectively? First, theory can help them _diagnose_ new situations as they arise. When seeking to address either a recurring issue or a specific new event, policy makers must figure out exactly what sort of phenomenon they are confronting. Is a stalemated negotiation due to lack of trust or are the protagonists simply too far apart to strike a bargain? Is an adversary seeking to alter the status quo because it is greedy, over-confident, or ideologically inspired, or because it is insecure and trying to enhance a weak position? By expanding the set of possible interpretations, theories provide policy makers with a broader set of diagnoses, and can help them avoid premature closure or dangerous forms of stereotyping.

Second, by identifying the central forces at work in the international system — what Kenneth Waltz called a “picture of a realm” — theory helps policy makers _anticipate_ future developments.\(^\text{16}\) This capacity is especially valuable when circumstances are changing rapidly and when straight-line projections from the past are unreliable. To take an obvious example, it would be foolish to try to forecast China’s future conduct by looking solely at its past actions, or even its recent behavior, because Chinese leaders are likely to revise their preferences as their relative power increases. A good theory,
however, could tell us how shifts in the balance of power will affect Chinese behavior and help leaders craft policies designed to forestall dangerous future developments.

Third, theory is essential to formulating policy prescriptions because all policy actions depend on at least some crude notion of causality. In other words, policy makers select measures A, B, or C because they believe they will produce the desired result. Theory helps policy makers select objectives, guides the selection of policy instruments, and identifies the conditions that must be met for these instruments to work.17

Fourth, theory is also critical to effective policy evaluation. In order to determine if a specific policy is working, policy makers must identify benchmarks measuring progress toward the stated goal(s). The selection of these benchmarks should be theoretically informed, based on what we think we know about the causal relationships involved in producing the desired outcome. Grand strategies based on realist theory tend to emphasize benchmarks that measure shifts in relative power, for example, while a strategy derived from liberal principles looks for increases in economic intercourse, levels of democratic participation, or the broadening and deepening of global institutions.

Finally, general theories of international politics can help us guard against various forms of chauvinistic stereotyping. In particular, realist theories highlight the importance of security in a world that lacks a central sovereign authority, and they highlight how structural forces will “shape and shove” even very different states in similar directions. Because they recognize that all states must rely on their own resources to defend themselves, realists are less prone to demonizing adversaries and less likely to see an opponent’s military preparations as evidence of aggressive intentions. Realists are also less surprised when the United States acts in ways that are at odds with its liberal values or its alleged commitment to advancing human rights because the theory depicts international politics as a competitive realm where even powerful states must sometimes compromise ideals in order to improve their security.

The Limited Impact of Theory

Although it is impossible to formulate policy without at least a crude theory (i.e., some notion of what causes what), even well specified theories of international relations do not seem to have much impact on policy formation. For starters, most theories of international relations seek to explain broad tendencies across time and space, omitting other variables that may be relevant for the specific case(s) that policy makers are grappling with at a particular point in time. None of our existing stock of theories has enormous explanatory power and the specific actions that states take are usually the product of many different factors (relative power, regime-type, individual leadership traits, etc.). Unfortunately, we lack a clear method for combining these various theories or deciding which one will exert the greatest impact in a particular case.

This problem is compounded by the broader context in which foreign policy is made. Social science works best when problems can be defined precisely and analyzed systematically; i.e., when actors’ preferences are known and fixed, when there is abundant data with which to test conjectures, and when the impact of alternative choices can be estimated precisely. This is rarely the case in the conduct of foreign
policy, however: actors’ preferences are often obscure, they usually have multiple strategies available, and the payoffs from different choices are often unknown. Non-linear relationships and endogeneity effects abound, and preferences and perceptions may change without warning. Even careful efforts to examine the impact of specific policy instruments, such as aid programs, economic sanctions or “foreign-imposed regime changes,” are rife with selection effects that make it difficult to estimate their causal impact.

To make matters worse, policy makers and theorists have very different agendas. Academic theorists pursue general explanations of recurring behavior, but policy makers are more interested in solving the specific problem(s) they face today. Although understanding tendencies can help policy makers understand whether their objectives will be easy or difficult, what happens “most of the time” is not as pertinent as knowing what is most likely to happen in the particular case at hand. Moreover, policy makers are often less interested in explaining trends than in figuring out how to overcome them. As a result, notes Arthur Stein, “in-depth experiential knowledge dominates general theorizing and statistical generalizations in the formation of policy.”¹⁸

Last but not least, the impact of academic theory is limited even more by the professionalization of the international relations sub-field and the growing gap between the Ivory Tower and the policy world. Although academics still migrate to policy jobs on occasion, their scholarly credentials do not win them much respect in official circles and may even be seen as a liability.¹⁹ They may also learn that politicians usually value loyalty and bureaucratic effectiveness far more than they prize academic distinction or theoretical novelty.

Moreover, like most political science, contemporary IR scholarship is written to appeal to other members of the profession and not intended for wider consumption, which is one reason why it is increasingly impenetrable and often preoccupied with narrow and trivial topics. Younger scholars understand that theoretical novelty and methodological sophistication are valued much more than in-depth knowledge of a policy area; indeed, there is a clear bias against the latter within contemporary political science. Those without tenure are routinely cautioned not to waste their time writing for policy audiences for fear of being deemed “unscholarly.” Because work that might be useful to policy makers brings few rewards, it is hardly surprising that university-based scholars rarely try to produce it.²⁰

Instead, the gap between theory and policy has been filled by the growing array of think tanks, consultants, and other quasi-academic groups that now dominate intellectual life in major world capitals, and especially in Washington, DC. Policy makers no longer need to consult university-based scholars for advice on pressing global problems, as there is no shortage of people inside the Beltway who are happy to weigh in and are being paid to do just that. These organizations can provide useful guidance, but there are obvious downsides to their growing prominence. Most Washington-based think

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Most Washington-based think tanks have an ideological agenda—usually shaped by their financial supporters—and their research output is subject to far less rigorous standards. They also lack the elaborate vetting procedures, including peer review, that universities rely upon to make personnel decisions. Policy makers can get outside advice that addresses immediate concerns but it is neither disinterested nor authoritative.

This is not to say that academic scholars have no impact at all. IR theorists occasionally provide the policy community and the wider world with a vocabulary that shapes discourse and may exert subtle effects on policy formation. Concepts such as “interdependence,” “clash of civilizations,” “bipolarity,” “compellence,” “soft power,” etc., form part of the language of policy debate, influencing decisions in indirect ways. Scholars can also exploit the protections of tenure to tackle especially controversial or taboo subjects, and may succeed in opening up debate on previously neglected subjects.

Yet in the United States at least, IR theorists rarely challenge taboos and rarely have much impact on policy unless they leave academic life and work directly in government themselves. Our collective impotence as a field should not surprise us: the United States is a very powerful country and its foreign policy bureaucracy is large, well-entrenched, and permeated by powerful interest groups and other stakeholders. It also has a system of divided government with many veto points, which makes policy innovation exceedingly difficult. Under these conditions, it would be fatuous to believe that a scholarly book or article—or even a whole series of them—could steer the ship of state in a new direction all by itself.

To have a significant impact on policy requires either direct involvement or sustained political engagement, activities that many academics are neither interested in nor well equipped to pursue. Back in the 1950s, for example, Albert Wohlstetter and his colleagues gave dozens of briefings presenting the results of the RAND Corporation’s famous “basing studies” in an ultimately successful effort to convince the military establishment to adopt their recommendations. Likewise, the neoconservatives’ protracted campaign for war with Iraq—which we now know was built on factual errors, biased analysis, and bogus theories—began in earnest in 1998, but did not bear fruit until five years later. Persistence, not perspicacity, is the real taproot of policy influence.

This situation has to be discomfiting to those of us who are both devoted to the “life of the mind” yet interested in using knowledge to build a better world. We can still hope to advance that goal through our teaching, and as previously noted, some scholars will have a direct impact through their own government service. There will be occasional moments when a scholar provides a new perspective or analytic approach that seizes the imagination of those in power, usually because it addresses the perceived needs of the moment. But for most members of the discipline, the goal of “speaking truth to power” will be an increasingly distant one.
Despite these limitations, academic scholars—including IR theorists—have at least three useful roles to play in the broader public discourse on international affairs. First, those who have thought longest and hardest about the nature of modern world politics can help their fellow citizens make sense out of our “globalized” world. Ordinary people often know a great deal about local affairs, but understanding what is happening overseas generally requires relying on the knowledge of specialists. For this reason alone, university-based academics should be actively encouraged to write for and speak to broader audiences, instead of engaging solely in a dialogue with each other.

Second, an engaged academic community is an essential counterweight to governmental efforts to manipulate public perceptions. Governments have vastly greater access to information than most (all?) citizens do, especially when it comes to foreign and defense policy, and public officials routinely exploit these information asymmetries to advance their own agendas. Because government officials are fallible, society needs alternative voices to challenge their rationales and suggest different solutions. Academic scholars are protected by tenure and not directly dependent on government support for their livelihoods, so they are uniquely positioned to challenge prevailing narratives and conventional wisdoms. For these reasons, a diverse and engaged academic community is integral to healthy democratic politics.

Third, the scholarly community also offers a useful model of constructive debate. Although scholarly disputes are sometimes heated, they rarely descend to the level of \textit{ad hominem} attack and character assassination that increasingly characterizes political discourse today. Indeed, academics who use these tactics in a scholarly article would probably discredit themselves rather than their targets. By bringing the norms of academic discourse into the public sphere, academic scholars could help restore some of the civility that has been lost in contemporary public life.

How might these miracles be accomplished? I have no illusions about creating some sort of philosopher kingdom where academics rule, and thirty years at three different universities and three different think tanks have convinced me that such a world would almost certainly not be an improvement. But should academic scholars of international relations really be proud that so few people care about what we have to say?

It will do little good to implore policy makers and the public to pay more attention to us; the only remedy is to produce work that is both academically rigorous, but also potentially useful to those charged with making policy decisions. What is needed, therefore, is a conscious effort toalter the prevailing norms and incentives in the academic community. This goal is not as far-fetched as it might seem, for these professional norms are neither fixed nor divinely ordained. Instead, the members of the discipline itself collectively determine the norms that govern our enterprise. As a largely self-policing community, we get to decide what traits we value most and there is no reason why policy relevance and public engagement could not be given greater weight.

\textbf{III. WHAT IS TO BE DONE?}

If the community of international relations scholars decided it was tired of being ignored, there are a number of practical steps that could encourage greater relevance. To wit:\textsuperscript{22}

Instead of focusing almost entirely on peer-reviewed professional journals and monographs, promotion committees could also conduct systematic evaluations of a faculty member’s contributions to broader public discourse. In addition to measuring citation counts, for example, review committees could also track news reports or blog hits referring to a candidate’s work. And instead of relying solely on evaluations from other scholars, these same committees could also solicit evaluations from policy makers working in the relevant domains. Discovering that a junior colleague’s work had exerted a major impact on how policy makers think about an issue is surely relevant to an evaluation of its long-term value.

2. Encourage Professional Associations to Honor Public Impact.

At present, the American Political Science Association gives dozens of awards for books, articles, and papers in various fields and subfields. It gives one award “in recognition of notable public service” and another for career achievement “to the art of government.” If we want to encourage scholars to aim for greater impact, creating one or two more awards designed to honor such achievements hardly seems excessive.

3. Make It Easier for Younger Scholars to Gain Policy Experience.

To encourage younger scholars to learn how the real world works, academic departments should make it easier for them to work in government or in other policy-relevant areas. For example, more universities could agree to halt the tenure clock if a junior faculty member wanted to spend a year working in government or for a non-governmental organization. This policy would create more scholars who actually knew how government worked and they would be more likely to produce work that would be accessible and relevant to policy makers. Because most students care about the real world and have limited interest in empty scholasticism, this policy would help create better teachers as well.


The ability to set and pursue one’s own research agenda is a key element of academic freedom. That principle should not be compromised, but academics should be more willing to listen to practitioners when deciding what subjects to explore. In addition to deriving new topics from the lacunae of existing scholarship, there is nothing to be lost from occasionally asking non-academics what sorts of knowledge they would like to have. We might be surprised by what good questions they come up with.

5. Convince University Administrations to Value Real-World Contributions.

Presidents, provosts, and deans can further these goals as well by rewarding departments whose members make substantial contributions to the public sphere and by withholding resources from those that are trapped in the “cult of irrelevance.” The purpose is not to encourage departments to devolve into platoons of headline-chasing policy analysts eager to hit the talk-show circuit, but rather to foster a more heterogeneous community at all levels of academia.

At present, professional ethics generally revolves around topics such as plagiarism, academic freedom, and abuses of power in the form of sexual harassment or the treatment of human subjects. These are important issues, but we should also encourage students to think long and hard about the debts that scholars owe the society that supports them and the question of whether we have a broader ethical responsibility to use our knowledge and training for the betterment of society. This discussion must also address the ethical pitfalls that can affect scholars who become directly engaged in policy-relevant research, and especially when funding or other sources of compensation are involved.

IV. CONCLUSION

As Keynes famously observed, “even the most practical man of affairs is usually in the thrall of the ideas of some long-dead economist.” It is possible that IR scholars exert a similar long-term impact but I am inclined to doubt it. Policy makers seem less and less interested in what we have to say, partly because they are too busy dealing with today’s problems, but also because we tend to pose questions they are not concerned with and we provide answers they do not think they need. If academics want to play a more active and constructive role in world affairs, in short, the content of our scholarship will have to change.

To encourage this shift, we will have to modify the criteria of merit within the discipline itself and give real-world relevance greater weight. Absent this change, we can expect the outside world to pay even less attention to what we have to say. Not only does this strike me as an abdication of our responsibilities as scholars and citizens but it bodes ill for the future of the discipline itself. For if we are not producing useful knowledge that can help society address common problems, why should students take our courses and why should universities continue to allocate scarce resources to our departments? Y

NOTES

2 Some of the materials in this lecture were later incorporated in Gordon A. Craig, Germany 1866–1945 (Oxford): Oxford University Press, 1978), pp. 479–495.
3 I was fortunate to study with both Alexander George (my undergraduate thesis advisor) and Kenneth Waltz (my dissertation chair). Their intellectual styles were quite different; George worked inductively and was wary of broad generalizations, whereas Waltz worked deductively and prized parsimony. Yet both saw social science theory as a tool to inform more intelligent and successful policies, and neither believed that academia should be an isolated community divorced from real-world concerns.
8 Strictly speaking, this argument lies outside the realist paradigm but is not inconsistent with realist arguments. Realists maintain that the pressure of anarchy encourages states to focus on advancing clear national interests because states will
pay a price if they pursue other aims. But realists also recognize that very powerful states can pursue non-realist goals if they are willing to pay that price, as the United States has done by giving Israel nearly unconditional support.

9 One sign of the more open discourse was the publication in 2012 of Peter Beinart’s *The Crisis of Zionism* (New York: Times Books, 2012). Beinart’s extended discussion of the Obama administration’s failed peace efforts also demonstrates that the lobby’s impact is undiminished.


15 Stephen Van Evera refers to such theories as being “prescriptive rich.” See Stephen Van Evera, *Guide to Methods for Students of Political Science* (Ithaca, NY: Cornell University Press, 1997). Alexander George advises scholars to “include in their research designs variables over which policymakers have some leverage.” See “Foreword,” in Miroslav Nincic and Joseph Leffgold, eds., *Being Useful: Policy Relevance and International Relations Theory* (Ann Arbor: University of Michigan Press, 2000). Theories that contain no manipulable variables can still be useful if they help policy makers understand the broader environment in which they are operating. For example, knowing whether the system is bipolar or multipolar can be valuable, even if one does not have the capacity to alter that condition.


17 This was the core objective of George’s work on “policy-relevant” theorizing, as well as the bulk of so-called middle-range theory, which tends to produce contingent generalizations about the impact of specific policy instruments like deterrent threats, economic sanctions, coercive diplomacy, etc. See his “Theory for Policy in International Relations,” in Alexander George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974).


19 Krasner offers a telling anecdote: “I did this once and it was really stupid. I said ‘I have a Ph.D. and I know about this.’ That’s a completely worthless comment. No one cares about your credentials... [being addressed as a professor in policy circles] was a bad thing because people thought you were a snob.” Quoted in Lisa Trei, “Does the Academic Study of International Relations Matter in the Real World of Policy?” *Stanford News Service*, January 27, 2003, at http://news.stanford.edu/pr/03/krasner129.html.

20 As Adam Przeworski noted a few years ago, “The entire structure of incentives in academia in the United States works against taking big intellectual or political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance.” Quoted in Gerald Munck and Richard Snyder, “What Has Comparative Politics Accomplished?” *APSA-CP Newsletter* 15, no. 2 (2004).

21 The RAND basing studies examined the question of how to deploy US bombers to optimize both their survivability in the event of a Soviet attack and their effectiveness in the course of the war. It is worth remembering that Wohlstetter was not a university-based academic at the time (though he later taught at the University of Chicago), but rather an employee of a government-sponsored think tank.

22 A more extensive presentation of these ideas can be found in Stephen M. Walt, “International Affairs and the Public Sphere,” *Social Science Research Council* (2011), available at http://publicsphere.ssrc.org/walt-international-affairs-and-the-public-sphere/

23 As Bruce Jentleson has written: “Should it really be the case that a book with a major university press and an article or two in a [refereed] journal... can almost seal the deal for tenure, but books with even a major commercial house count so much less and articles in journals such as Foreign Affairs count little if at all?... [T]he argument is not about padding publications counts with op-eds and other such commentaries, but it is to broaden evaluative criteria to better reflect the type and range of writing of intellectual import.” See his “In Pursuit of Praxis: Applying International Relations Theory to Foreign Policymaking,” in Nincic and Leffgold, *Being Useful*.  

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